the Late Bronze Age. The All Cannings Cross type pottery (to which the Brean pottery from this layer 4 was ascribed) is itself now dated to the eighth–seventh centuries BC at Longbridge Deverill, Cowden (Cunliffe, 1974). Apsimon agrees (pers. comm.) that his layer 4 should be redated to Late Bronze Age.

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CUNLIFFE, B. W. 1974. *Iron age communities in Britain: an account of England, Scotland and Wales from the seventh century BC until the Roman Conquest* (London).

The apparently unfinished nature of the bracelets and the fragment of metal cut from a similar thickness of gold band is of great interest, suggesting that a goldsmith was working on the site and that the objects were concealed within the settlement where he was working.*

* In November 1983 the Coroner's Court at Burnham declared all three objects Treasure Trove.

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HALDANE, J. W. 1969. A gold bracelet from Hope Wood, Wookey Hole, Somerset, *Proc. Somerset. Archaeol. Natur. Hist. Soc.*, CXIII, 99–101.

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PROUDFOOT, V. B. 1955. *The Downpatrick gold find.* Archaeological Research Publication (Northern Ireland), No. 3.

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Archaeological Review from Cambridge

We have already (*Antiquity*, 1982, 166) drawn attention to the *Oxford Journal of Archaeology:* now, in all Oxbridge fairness, we draw attention to a light-blue publication, *Archaeological Review from Cambridge,* produced in the Department of Archaeology, Downing Street, Cambridge CB2 3DZ: it is not an official departmental publication and doesn't seem to have an Editor, but an Editorial Committee of ten led by Gina Barnes and Sheena Crawford. It is published biannually, spring and autumn, and the rates for UK subscribers are £4.00 (individuals) and £6.00 (institutions); overseas subscribers £5.00 (individuals) and £7.50 (institutions). We are told that this is 'a journal conceived and produced by students of the Department of Archaeology in Cambridge. *ARC* was first published in 1981 and has returned to production . . . following a temporary hiatus.' The Spring 1983 issue was devoted to Archaeology and the Public: it had 80 pages—ten articles, commentary, correspondence and reviews of five books. There are some good and economically written articles; let us invidiously mention a few: Tony Gregory on the impact of metal detecting on archaeology and the public, Bruce Norman on archaeology and telesion, Tony Williamson and Liz Bellamy on ley-lines, and Christopher Chippindale on Stonehenge, General Pitt-Rivers and the first Ancient Monuments Act. All good luck to a good journal.

Relict Neanderthals : a folkloristic comment

The 'comment' in the title of this note is upon Dr Myra Shackley's article in this journal: 'The case for Neanderthal survival: fact, fiction or faction?: (LVI, 1982, 31–41). Mr Robert E. Walls thinks she is wrong not to give due consideration to the importance and implications of 'wild man' folklore. Mr Walls, while an anthropology student at the University of Washington, has recently completed a lengthy thesis on a cross-cultural examination of North American 'wild man' traditions, and is hoping shortly to pursue his studies by attending the Indiana University Graduate Folklore Program. He writes, at present, from 4601 Evanston No., Seattle, Wa. 98103, USA.

In a recent issue of ANTIQUITY (March 1982), Myra Shackley presented her case for the survival of Neanderthals in remote mountain areas of southern Russia and Mongolia. It is Shackley's contention that the evidence adduced 'amply demonstrates' the biological existence of these 'unclassified hominoids' (who may be responsible for some high altitude Mousterian assemblages), despite the absence of skeletal remains. While assertions of this kind are neither new nor unique—Russian and North American scholars having researched this topic energetically over the past 25 years—they remain, to say the least, highly controversial. As a folklorist who has investigated, quite intensively,

the cultural history of 'wild men' in North America (Walls, 1980), I would like to make a few comments pertinent to this problem.

First, I am rather surprised that Shackley neglected to mention several important references particularly relevant to her discussion. In 1978, a conference was convened at the University of British Columbia to discuss 'Sasquatch and Similar Phenomena'. The resulting collection of papers (Halpin & Ames, 1980) provides objective assessments from scholars representing both the social and natural sciences, and includes one linguist's analysis (Colarusso, 1980) of a Caucasus 'wild man' belief. Neither does Shackley refer to the work of A. Z. Rozenfeld (1959) who, based on his own fieldwork in the Pamirs, detailed the folkloric elements present in Pamir 'wild man' traditions, and came to the conclusion that such beings are strictly 'mythological'.

Secondly, these oversights indicate a considerable problem. I do not think that all orthodox archaeologists will quarrel endlessly with Shackley's belief that some Neanderthals *could* have survived to coexist with modern man, provided they did have a remote region to maintain a breeding population. But she also asks us to consider her argument scientifically plausible on the basis of specific 'positive evidence', when on the other side of the world, in North America, the same concatenation of evidence occurs in the form of another unknown hominoid—the Sasquatch. Yet Shackley avers that Almasti are quite distinct from 'general Bigfoot mythology', and not 'part of the general run of Yeti . . . or Sasquatch . . . legends.' This is clearly an unsupportable position.

Evidence for both Almasti and Sasquatch includes numerous eyewitness accounts, footprints which are not obvious fakes, reported captives and corpses, geographical place names, possible paleontologically established antecedents (*Gigantopithecus* for Sasquatch), and a very mundane type of folklore which depicts the creatures as scientifically credible.

Why might this evidence seem so similar? In both areas the borrowing, assimilation and transformation of various appealing traditions, held by either the native or non-indigenous population, is a possible and likely consequence of post-contact situations. For example, if Native Americans of the Pacific Northwest coast can borrow the simian image from Europeans to the extent that some traditional beings are now called and said to look like 'apes', 'gorillas' or 'wild men', it is not wholly unreasonable to suppose that Mongol, Kirghiz and Kazakh herdsman might have done the same.

On the other hand, the cultural contexts of the anthropomorphic images of Almasti and Chuchunaa bear a close resemblance to several categories of traditional entities known to Native American groups over a widespread area: namely Northern Athapaskan 'Bushmen', Algonkian 'Windigo' (or 'Witiko'), and Salish 'Stick Indians'—beings which today are often identified as Sasquatch. All are humanoid, live in remote areas or on the fringe of human settlement, and, most importantly, embody physical and behavioural characteristics which are distinctly non-human. These characteristics symbolically represent each culture's antithesis of sanctioned behaviour and appearance (see Basso, 1978; Preston, 1980; Suttles, 1972). Further research into the Almasti-Chuchunaa image may reveal a similar pattern of social utility.

Over time, anthropologists have detailed just how rich the conceptualization of the hominoid figure is in the mythopoeic minds of all cultures. Out of these figures, interest in recent years has focused upon a very few which are more mundane and less fantastic than the rest, and hence somewhat credible in terms of the Western scientific tradition. Evidence from both paleontological and mythological pasts has been offered as confirmation for the present biological existence of 'wild men'. This does not take into account, however, the fact that greater numbers of people around the globe are now becoming more aware of those pasts. And that awareness is effectuating changes in their folklore, sometimes altering traditions in such a way as to make them more tenable to the inquiring collector. The process by which this change in folklore occurs is evident in contemporary 'wild man' traditions from China, Russia, and North America, and its dynamic force, I believe, is an attempt to come to terms with the psychological dilemma posed by the recognition of rapidly shrinking wilderness areas and natural resources.

Ultimately, Shackley's argument rests on the unsupportable premise that because of its unique realism and 'lack of mythological overtones' (*vis-à-vis* other 'wild men'), the Almas deserves special attention. I must respectfully disagree with Shackley, because her argument fails to consider judiciously contemporary 'wild man' folklore from a broad, comparative perspective. Until skeletal remains are found, the folklorist's work must not be

ignored, especially if folklore is going to influence the interpretation of Mousterian artifacts.

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COLARUSSO, J. 1980. Ethnographic information on a wild man of the Caucasus, in Halpin & Ames, 255–64.

HALPIN, M. & M. AMES (eds). 1980. *Manlike monsters on trial: early records and modern evidence* (Vancouver).

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ROZENFELD, A. Z. 1959. O nekotorykh perezhitkakn drevnik verovanii u pripamirshkikh narodov, *Sovetskaya Etnografiya*, 4, 55–66.

SUTTLES, W. 1972. On the cultural track of the Sasquatch, *North-west Anthropological Research Notes*, 6(1), 65–90.

WALLS, R. E. 1980. Toward an anthropology of Sasquatch: native and Euro-American wild man beliefs and traditions. Unpublished Honors Thesis, University of Washington, Seattle.

Fieldwork publication and the CBA/DoE report

We asked Dr Robin Derricourt to give us a report of the Microform in Archaeology Conference held in Oxford in the autumn of 1981. At his suggestion we agreed to await the publication of the important report of the CBA/DoE Committee on Publishing in Archaeology so that he could link these various matters in his note for us. He would like us to stress that it is more in the form of a personal comment than straight rapportage, and also that it is in no way any kind of official reaction from the Cambridge University Press where he works, as their rapidly growing archaeological list bears witness, as Associate Director, Humanities and Social Science.

The expansion of field research in the United Kingdom over two or three decades is well known as a trend, a challenge, as a problem of data handling and presentation. Major research and 'rescue' programmes accompanied rural motorway and pipeline schemes, urban replanning and rebuilding, and local government reorganisation and initiatives. The backlog of post-excavation work, and the accompanying difficulties of making available results through formal publication and other means, have become dominant issues, to individual archaeological units and their official funding sources. A new joint report by the Council for British Archaeology and the Department of the Environment represents a bold new approach to this problem.

The methods and organization of fieldwork have adapted to match the challenges of extended work. If British archaeological field and excavation methods always attracted the admiration and emulation of many continental and trans-Atlantic colleagues, the increased sophistication of methods of survey, stratigraphic excavation, recording and interpretation continue to make British archaeologists universally admired, as innovators and craftsmen in the field.

This makes the more remarkable and striking the contrast between the 'sharp end' of fieldwork and the backward-looking nature of what most archaeologists still accept as both goal and responsibility in field research: the 'publication' of results, however that may be defined. Formal publication itself is often bypassed—a phenomenon even more true of the United States, as Colin Renfrew has recently observed in *American Antiquity* 48(1) (1983). The information revolution, almost as much a phenomenon of the human sciences as the natural sciences, sometimes appears to have bypassed archaeology.

This is still reflected, informally, by the archaeologist whose team's research methods have a ten-fold sophistication over those of a generation back, lovingly caressing a pre-war Antiquaries report as the goal of proper publishing.

Faced with the expansion of data and the problem of adequate publication, the Ancient Monuments Board appeared, eight years ago, to be grasping the issue. But the 1975 'Frere report', *Principles of publication in rescue archaeology*, often seems to have been observed more in trite quotation than action. It made the positive distinction between the site archive 'available for inspection', the full illustrated and descriptive publication 'in journal *or* occasional papers, *as required*, available as duplicates, microfiche, microfilm *or* computer print-out' (my emphases), sufficient alternates for any and every preference or none; supplemented by the 'publication in multiple copies of synthesis reports'. This muddled conclusion blurred some detailed and thoughtful arguments in the report's text, which led to the criticism of excavation reports outside proper monograph series or established journals, a view widely ignored. The Council for British Archaeology *Research Reports* represent an honourable and professional exception to the 'informal' publication of results. The pressure for official Government funding of full-length publication, which followed the Frere report,